THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

66TH STREET AND YORK AVENUE
NEW YORK 21, N.Y.

March 6,1955

Dr. Joshua Lederberg
Dept of Genetics
University of Wisconsin
Madison, Wisc.

Dear Joshua:

Since I received your letter I have spoken to Alan and he told me that he had written to you that he had changed his mind about going to Wisconsin, so there is nothing further I might add.

I've met Miss Thomas and she's asked me about Wisconsin. Since she is only a beginning graduate student I assume you are interested in her personality rather than any scientific achievment. I have found her a very pleasing person and well adjusted. She has very little in the way of academic background but seems willing and able to learn. I'm not sure what you mean by"fit in" but if it refers to being troublesome I would tend to doubt it.

I have intended to write you in detail about some of the work that Alan and I have been doing but every time I tried, it started to turn into a manuscript. As Alan mentioned we have been attempting to write things up and as usual this turned out to be a most enlightening experience in separating fact from fancy and so we are back in the laboratory giving some basis in fact for some wilder speculation. In essence what we are trying to show is that the genetic material of bacteriophage has homologous material in the sensitive non-lysogenic bacterium and that lysogenization may be only an allelic shift or at best the addition (non-substitutive ) of a small fragment. We tried to show this solely by radiological means but this was only supportive and not conclusive, so we are now looking for direct evidence of bacterial and phage recombination. Believe it or not it looks as if we just fell into some. Since everything hinges upon the different connotations of the same words I shan't endeavor to explain at this time. Ruth Sager mentioned your coming to NY some time this spring I hope to see you then and we can have a good discussion.

The only reason I wanted a gal- that gave stable transducions was that I feared the unstable might be insufficient for detailed quantitation. Papillate selection seems pretty good in most cases I have studied but I've never looked at one continually segregating and I was afraid one might lose transductions for rather trivial reasons with unstable systems (medium differences &c). The simple straight forward experiment is exactly what I intend to try with a slight variation. A small dose of UV to the host to mitigate lysogenization and get complete lysis. My own hunch is that it won't work in systems that show already lytic activity but then I have different preconceptions. Upon receipt of the stocks will try it both with Salmonella and coli.

I do not have any coli stocks and although I might have gotten them from CSH I preferred to get them from you so that you both knew that I had them and also had some idea what I was going to do with them.

Regards to all;

Yours sincerely,

Norton